Delayed primary school enrollment in Latin America[*]

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Introduction

The Millennium Development Goals specify that children in every country should be able to complete a full course of primary schooling by 2015 (Birdsall et al. 2005). Judged exclusively by net primary enrollment rates, many Latin American countries appear close to meeting this goal (Inter-American Development Bank 2005). Yet, this belies an important pattern among younger children. To wit, net enrollment rates often peak *after* the age of 6 or 7, leading to an "inverted-U" pattern in age-enrollment profiles (Urquiola and Calderón 2006). A plausible interpretation is that many children delay enrollment, even when all children have access to a primary school.[1]

Delayed enrollment provokes special concern because it might result in fewer years of schooling attained, especially among poor children (Urquiola and Calderón 2006). Suppose that poor families delay children's enrollment in order to postpone incurring the direct costs of primary schooling, such as uniforms, textbooks, or transportation (Tsang 2002). The value of children's time in the labor market rises with age and schooling, suggesting that, all else equal, late enrollment places older children at greater risk of dropping out and working. Some authors use delayed enrollment as a rough indicator of school or student failure.[2] But, in general, the theoretical and empirical literature that explains the causes of delays is sparse, and limited to African and Asian countries.

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[1] There is limited evidence from high-quality longitudinal or retrospective data on children's exact age of primary school enrollment in Latin America. McEwan and Shapiro (forthcoming) report estimates from longitudinal student data in Chile. This paper reports estimates from retrospective responses in Guatemalan and Panamanian household surveys. Outside Latin America, a larger literature has documented high rates of delayed enrollment, especially in Sub-Saharan Africa, using retrospective data from household surveys (Glewwe and Jacoby 1995; Partnership for Child Development 1999; Bommier and Lambert 2000; Glewwe et al. 2001; Alderman et al. 2006).

[2] Patrinos and Psacharopoulos (1995; 1997) use age-grade distortion— a child's actual age in a primary grade, relative to her grade-appropriate age—as an indicator of outcomes in Bolivia, Guatemala, and Peru. The indicator combines the incidence of delayed enrollment and subsequent grade repetition.

This paper makes four contributions to this small literature. First, it derives estimates of mean enrollment age in eight Latin American countries. Towards doing so, it identifies household surveys that include exact day of birth. Using these data to calculate exact decimal ages, instead of commonly available integer ages, allows for precise cross-country comparisons of consistently defined cohorts of young children. The paper confirms a common anecdotal impression: that primary school enrollment ages tend to be higher in poorer countries, and among poorer families within countries.

Second, the paper reviews the limited theoretical and empirical work on the phenomenon of delayed enrollment. Drawing upon Glewwe and Jacoby's (1995) seminal work, it describes two broad explanations for delays consistent with the previous evidence. The first is that delays are the voluntary (and rational) response of parents. Specifically, parents may observe that their child lacks "readiness" for primary schooling—whether physical, cognitive, or social—and decide that costly delays are justified by the potential benefits of greater learning. The second is that delays are the involuntary, constrained response of poor families who are unable to finance the costs of "free" primary schooling. Empirical evidence from Africa suggests that malnourished children are most likely to delay, even controlling for socioeconomic status, providing some support for the first view (Glewwe and Jacoby 1995; Alderman et al. 2006).

Third, a multivariate analysis of the causes of enrollment delays provides support for the view that lower school readiness—proxied by malnutrition, speaking a language other than Spanish, or preschool attendance—is consistently linked to delayed enrollment. There is less consistent evidence that family income or wealth proxies are linked to delays.

Fourth, the paper describes four categories of policy interventions in Latin America and related empirical evidence on their impacts. The most promising of these—if the goal is to reduce enrollment delays—focus upon improving school readiness and skills among very young children (e.g. early childhood stimulation and nutrition),[3] or children attending early primary grades (e.g. remedial tutoring and bilingual instruction).[4] Often, participation in such pre-primary and primary interventions can be encouraged via conditional cash transfers (Rawlings and Rubio 2005), which have the salutary effect of diminishing poverty-related constraints that prevent on-time enrollment. The least promising policies focus upon the construction of additional schools.

Facts about Primary Enrollments

Age-Enrollment Profiles

Household surveys from eight Central and South American countries include children's exact day of birth (see Table 1). With these data, I calculate children's *decimal* ages on January 1 of the survey year, which permits cross-country comparisons of consistently defined age cohorts.[5] I further calculate two binary measures of schooling: (1) whether a child attends primary school and (2) whether a child attends pre-primary (Table A1 reports country-specific details).

Figure 1 illustrates, in each country, the weighted proportion of children attending school at each age (i.e. the net enrollment rate). The solid and dotted lines show the proportions attending primary and pre-primary school, respectively.[6] Each panel includes vertical lines at ages 6.0 and

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[3] See Schady (2006) for a review.
[4] See Anderson (2005) and Winker (2000).
[5] The advantages of using decimal ages, instead of self-reported integer ages, are twofold. First, survey collection dates vary widely across countries (see Table 1), so one country's "6 year-olds" may be older or younger than another's. Second, integer ages are not useful for assessing compliance with precise enrollment age guidelines. Several countries define minimum ages in decimal years, using a birthday cutoff date. For example, Argentine and Chilean students must turn 6 by June 30 in order to begin the first grade in March, implying a minimum enrollment age of 5.5 on January 1 (McEwan and Shapiro forthcoming; Ministerio de Educación, Cienca, y Tecnología n.d.).
[6] Because of small samples, proportions taken within decimal age cells yield noisy enrollment-age profiles. To partially smooth the profiles, I estimated local linear regressions (bandwidth=0.03) of enrollment proportions on

8.0 to focus attention on cohorts of young children. Overall, the figure illustrates four facts about net enrollment rates. First, the vast majority of children attend primary school at some point (Inter-American Development Bank 2005; Urquiola and Calderón 2006). Even so, two of the poorest countries (Guatemala and Nicaragua) still have proportions that rarely exceed 0.9 at any age.

Second, the net enrollment rate increases gradually at lower ages, consistent with delayed primary school enrollment, and usually reaches its highest point after the age of 7. There are countries, notably Argentina and Panama, in which enrollments increase sharply before the age of 6, suggesting that families are enrolling children at, but not below, administratively prescribed ages. In Argentina, for example, children must turn 6 on or before June 30 of the academic year in order to enroll in the first primary grade, implying a minimum enrollment age of 5.5 (on January 1). Indeed, the net primary enrollment rate jumps sharply at that age.

Third, a few countries have unexpectedly large proportions of "early" primary enrollments (i.e., at ages before 6). In Bolivia, first-grade students are supposed to turn 6 on or before March 31 of the academic year, implying a minimum enrollment age of 5.75 (on January 1).[7] Unlike Argentina, however, the rule is apparently not enforced. Among Bolivian children ages 4.75 to 5.75, the primary enrollment rate is already 0.44.

Fourth, pre-primary enrollment rates in most countries are comparatively low, with a few exceptions. For example, Argentina's relatively high kindergarten enrollment is the result of recent investments in kindergarten infrastructure (Berlinski et al. 2006; Berlinski and Galiani 2007).

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age, and then predicted values for the proportion in each age cell. The plotted proportions are the fitted, smoothed values from these non-parametric regressions.

[7] See Ministerio de Educación (2001).

Estimating the Mean Enrollment Age

Table 2 reports net primary enrollment rates within integer age categories, based upon the same data as Figure 1. For example, 0.9% of Argentine 4 year-olds and 48.8% of 5 year-olds attend primary school, a between-year increase of 47.9% (the increase is reported in parenthesis). In lieu of precise data on each child's age of enrollment, I use net enrollment rates to estimate the mean enrollment age. For Argentina, I calculate a weighted average, assuming that 0.9% of 4 year-olds—as measured on January 1—enroll at an average decimal age of 4.5, an additional 47.9% at 5.5, 49.2% at 6.5, and so on.[8]

The estimates, though imperfect,[9] confirm anecdotal impressions of enrollment age. Argentina's mean (6.03) is consistent with an official enrollment age window of 5.5 to 6.5. Bolivia and Paraguay have surprisingly low enrollment ages—below 6—though both countries have wider distributions that reflect larger proportions of both "early" and "delayed" enrollments. Of the four countries with the highest means (Brazil, Guatemala, Nicaragua, and Peru), three have an official enrollment age of 7 years old (see Table 1). Guatemala, as in many education and health indicators, stands out in the Western Hemisphere for its poor performance (World Bank 2004; McEwan and Trowbridge 2007).

Income and Enrollment Age

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[8] In a most cases, net enrollment rates eventually decline between years. To avoid negative weights, I assume that the remaining proportion of children (the difference between 1 and the sum of previous weights) do not enroll, and that their enrollment age is right-censored. These proportions are only appreciable in Guatemala (0.13) and Nicaragua (0.08).

[9] First, the estimates overestimate the mean age if the changes in age-specific enrollment rates cannot be entirely attributed to new enrollments (that is, a gain of 0.3 could reflect an even larger proportion of new enrollments, balanced by early drop-outs). Second, they underestimate the mean age if older, right-censored children eventually enroll in school.

Figure 2 plots the mean enrollment age of each country against its Gross Domestic Product per capita.[10] The scatterplot suggests that richer countries enroll children at younger ages ($r = -0.4$), despite outlying observations from Bolivia and Paraguay. I further assess how children's enrollment age is related to family incomes *within* countries. Since surveys do not report comparable measures of exact enrollment age among young children, I focus upon the primary school attendance of young children, presuming than non-attending children are "delayed." I divide each country's sample by quintiles of family income per household member.

Table 3 reports net enrollment rates within country-by-quintile cells. With the exception of Bolivia, they suggest that children in the lowest quintile are less likely to attend primary school at younger ages than children in the highest quintiles. The differences are especially stark in Nicaragua and Guatemala, poorer countries that already have lower absolute enrollment rates.

Theory and Empirical Evidence

Why Do Families Delay Enrollment?

Why do some children, especially those in poorer countries and families, enroll at older ages? The theory of Glewwe and Jacoby (1995) emphasizes two likelihoods: (1) that children in some families are less "ready" for school, and *rationally* delay enrollment, or (2) that children in some families are constrained by poverty, and are *forced* to delay enrollment. To develop these ideas, Glewwe and Jacoby posit that families of school-eligible children, in deciding whether to delay enrollment, weigh its potential costs against benefits.

[10] Figure 2 includes additional observations from Zimbabwe (Alderman et al. 2006) and Chile (McEwan and Shapiro forthcoming).

The potential costs of delayed enrollment to children and parents include fewer years of schooling attainment,[11] shorter working careers,[12] and the value of lost child care opportunities.[13] The potential benefits are less clear-cut, though researchers in psychology hypothesize that older children become increasingly "ready" for primary schooling, and thus capable of learning more quickly (Stipek 2002). School "readiness" could encompass many child attributes, including physical well-being, cognitive skills, and language development. Mounting empirical research concludes that delaying enrollment in kindergarten or first-grade by one year produces large reductions in grade repetition and large increases in test scores that persist until at least the eighth grade (Bedard and Dhuey 2006; Datar 2006; Elder and Lubotsky 2006; McEwan and Shapiro forthcoming).[14]

Consistent with this empirical evidence, Glewwe and Jacoby's (1995) theory assumes that relatively older children—presumably more "ready" upon enrollment—will eventually reap higher lifetime earnings. In such a case, a voluntary enrollment delay and the concomitant gain in readiness *could* yield benefits in excess of costs for some children. If benefits exceed costs, then voluntary delays function as a rational and tacit investment by families in schooling

[11] Less schooling implies reductions in lifetime income (Patrinos and Psacharopoulos 2004). In developing countries, the likely source of lower attainment is the rising value of older children's time—in the labor market or family business—and the concomitant incentive for poverty-constrained families to withdraw older children from school before reaching a specified grade (Urquiola and Calderón 2006).

[12] Glewwe and Jacoby (1995), using Ghanaian data, conservatively estimate that a year's delay could lower lifetime wealth by 6%. Their simple estimates assume that education attainment and other outcomes are unchanged.

[13] Families give up an implicit child care subsidy when their child does not enroll in school. For some families, this lowers income by forcing a parent, usually the mother, to stop working or reduce hours of work. Consistent with this notion, research in the U.S. (Gelbach 2002; Cascio 2006) and Argentina (Berlinski and Galiani 2007) finds that maternal employment is responsive to availability of kindergarten slots.

[14] These papers must address a methodological challenge, since enrollment age is potentially correlated with unobserved characteristics of children and their families (e.g. family wealth) that also affect student outcomes. Most authors circumvent this form of selection bias by comparing "delayed" and "non-delayed" children that were *obligated* to do so by virtue of their birthday, relative to the enrollment cutoff date. Specifically, families born in months just before cutoffs (e.g. Chilean students born in June) tend to enter school at a younger age than students born just after cutoffs, in July. Presuming that birth timing is "as good as random assignment," this provides a natural experiment to assess the effects of enrolling at an older age. In Chile, a one-year delay improves fourth and eighth-grade grade test scores by at least one-third of a standard deviation (McEwan and Shapiro forthcoming).

readiness. In such cases, government policy responses could emphasize alternative investments—including early nutrition and preschool—that improve children's readiness while reducing voluntary (and costly) delays.

Suppose instead that costs outweigh benefits, but that constrained families delay enrollment because they are *forced* to do so. A first constraint is related to family income and wealth, which may be insufficient to finance family subsistence as well as new investments in children's primary schooling. In Latin America, the costs of "free" primary schooling—such as uniforms, textbooks, and matriculation fees—are frequently a large proportion of poor households' consumption.[15] The same families may be constrained in their ability to obtain credit, given lack of collateral or missing credit markets. A feasible alternative is to defer primary schooling, while saving for a child's eventual enrollment. The policy responses to income- and credit-constrained families include reducing the direct costs of primary schooling or even providing cash payments to families, perhaps conditioned on enrolling children "on-time." The use of so-called conditional cash transfers has recently assumed a key role in the education policies of several Latin American countries (Rawlings and Rubio 2005).

A second constraint is related to the supply of school opportunities. Suppose that families wish to enroll a child in the first grade, but that limited slots are rationed to relatively older children. This is less likely in Latin America, where supply constraints in the early primary grades have been mostly overcome (Urquiola and Calderón 2006). But, in such cases, policy responses should emphasize the expansion of primary school supply.

A third constraint is related to rules that govern the official age of enrollment. There are few constraints on maximum enrollment ages, but some families may wish to enroll children at ages younger than a promulgated minimum, if costs of waiting one year are larger than the benefits of

[15] See Tsang (2002), especially the references in Table 7.1.

doing so. Section 2 suggested that such constraints are mainly evident in Argentina and Panama. In other countries, such as Bolivia, the minimum is frequently violated. One explanation consistent with this evidence is that families do not have access to pre-primary schooling, and value the implicit child care subsidy of primary schooling.

Empirical Evidence

A small literature, mainly in African countries, estimates the determinants of children's enrollment ages. Its frequent goal is to distinguish among the previous theoretical explanations of enrollment delays. Glewwe and Jacoby's (1995) own empirical analysis finds that lower height-for-age Z-scores among Ghanaian children are strongly associated with enrollment delays. The Z-score indicates the difference, in standard deviations, between a child's height and that of a median child in a healthy population of the same age and sex. It is commonly employed as a measure of stunting caused by chronic malnutrition. They interpret the results as strong evidence that children's physical readiness, or lack thereof, induces parents to voluntarily delay enrollment. The authors find some evidence that family income, though not school fees, are associated with delays, providing mixed support for an income- and credit-constraints explanation. They find no evidence that school supply constraints cause delays.

A strong caveat to the causal interpretation of these findings is the potential correlation of the independent variables with unobserved variables that are the true cause of delays. In the previous example, child malnutrition could serve as a proxy for the (unobserved) poverty of families or local communities. As such, the results may be consistent with any (or all) of the theoretical explanations described above. To address this critique, the authors attempt to identify exogenous or "random" variation in child height that is caused by instrumental variables (IVs) such as distance to local health facilities (for a discussion of the approach, see Ravallion 2005). The approach is only valid if the proposed IVs are correlated with child malnutrition, but *uncorrelated* with unexplained variation in enrollment delays. Yet, family distance to health facilities may itself be a good proxy for unexplained determinants of enrollment age, such as poverty and wealth.

In Zimbabwe, Alderman et al. (2006) also find that height-for-age Z-scores are negatively associated with enrollment age.[16] To allay prior concerns about bias, the authors compare how *differences* in siblings' height-for-age affect *differences* in enrollment ages (thus controlling for all variables that are constant within families, such as income, wealth, and local schooling availability). In additional estimates, the authors combine the sibling-difference strategy with instrumental variables, finding robust results. The IV estimates rely on intra-family variation in child height-for-age that is induced by differential exposure of siblings to periods of drought and war.

Lacking data on child health, other authors have focused upon the role of family income, wealth, and schooling costs in determining enrollment age.[17] In Tanzania, Bommier and Lambert (2000) find that distance to school, a proxy of non-monetary schooling costs, and household expenditures are correlated with enrollment age. Ainsworth et al. (2005) find that Tanzanian children between 7 and 10 years old are less likely to attend school—approximating delayed enrollment—when a parent has died or when households are less wealthy.

Only one study uses data from either Latin America or a randomized experiment (Behrman et al. 2005). Poor Mexican families were randomly awarded cash transfers, conditional on greater

[16] A study in the Philippines, using a variant of this empirical strategy, yields similar conclusions (Glewwe et al. 2001). Another paper corroborates this finding in samples of Ghanaian and Tanzanian children, though it makes fewer statistical controls for child and family characteristics (Partnership for Child Development 1999).
[17] Behrman and Knowles (1999) find that enrollment age in Vietnam is associated with income, as in section 2 of this paper, although the analysis controls for few other variables.

child participation in school (Rawlings and Rubio 2005; Skoufias 2005). The transfers had little apparent effect on Mexico's already high rates of attendance among younger children in primary school, but did significantly reduce grade repetition and drop-outs.

The most consistent finding in the extant literature is the importance of early childhood nutrition in determining enrollment age. Even so, alternative measures of child readiness for primary school—such as pre-primary attendance or early cognitive skills—are typically not available to researchers. Family income and school costs have mixed effects that depend on context and model specification. Finally, there is a scarcity of empirical research in countries outside of Africa.

Method and Data

Empirical Approach

I first report estimates of the following regression:

(1) $$Attend_{ij} = \beta_0 + C_{ij}\beta_1 + H_j\beta_2 + \varepsilon_{ij},$$

where $Attend_{ij}$ is a dummy variable indicating whether child *i* in household *j* currently attends primary school, regressed on vectors of child-specific and household-specific variables (C_{ij} and H_j, respectively). ε_{ij} is an error term. The constant and slope coefficients are estimated via ordinary least squares (OLS), such that the estimates of β can be interpreted as marginal probabilities (maximum-likelihood probit estimates yield very similar results and are not reported here). To account for the nesting of children within households, the usual standard errors are adjusted for clustering at that level (Wooldridge 2002).

The dependent variable is intended to reflect the extent of enrollment delays, though some non-attending children may have already enrolled but then dropped out early. To reduce this possibility, I restrict the sample in each country to children with integer ages of 6 and 7 (calculated on January 1 of the survey year in each country). This does not necessarily correspond to the official enrollment age. In particular, it includes children in some countries that are younger than the official enrollment age (especially in Nicaragua, Guatemala, and Brazil). However, the descriptive evidence suggested that many families are relatively unconstrained by such rules.

A second set of analyses relies on an alternative measure of first-grade enrollment age, only available in the household surveys of Guatemala and Panama. Individuals report the integer age at which they enrolled in the first grade, or whether they have yet to enroll.[18] For individuals that are still not enrolled, I code a right-censored age (adding one year to their current age). With this dependent variable, I estimate

(2) $$Age_{ij} = \beta_0 + C_{ij}\beta_1 + H_j\beta_2 + \varepsilon_{ij}\,.$$

The virtues of using the alternative dependent variable are twofold. First, it allows the analysis to include a larger sample of primary-aged children (ages 6 to 11), allowing more statistically precise estimates. Second, the larger sample includes many children that reside in the same household. As such, I can estimate a variant of equation (2) that includes fixed effects for each household (α_j), following Alderman et al. (2006) and Glewwe et al. (2001):

(3) $$Age_{ij} = \beta_0 + C_{ij}\beta_1 + \alpha_j + \varepsilon_{ij}\,.$$

The inclusion of household fixed effects controls for variables, both observed and unobserved, that are constant within households. A weakness of the approach is that it prevents separate

[18] In Guatemala, the question posed is "How old was … when s/he *registered* the first time in first grade?" In Panama, it is "How old was … when s/he *enrolled* for the first time in first grade?"

coefficient estimates for household-specific variables (like income), since these are absorbed by the household fixed effects.[19]

The initial estimates of equation (3) employ OLS. However, two aspects of the dependent variable require special attention. First, the ages are discrete, interval-coded versions of exact decimal age. Second, some children have yet to enroll in primary school, and enrollment ages are right-censored, mainly in Guatemala. To account for both issues, I estimate a variant of equation (3) with interval regression, which is simply an ordered probit with fixed cut points (Wooldridge 2002).[20] A benefit of this method is that coefficient estimates are still directly interpretable as marginal effects (as it turns out, results are not very sensitive to the method of estimation).

Independent Variables and Samples

I define a common set of child and household variables across eight household surveys, using the harmonized data sets maintained by the Inter-American Development Bank. The main child variables include binary indicators of whether a child lives in a rural area, the child's gender, and whether a child belongs to a linguistic or racial minority.[21] To measure the last variable, I identified students whose self-reported dominant language is Spanish (or, in Brazil, a self-reported racial category, "white"). This is the omitted category in multivariate analyses.

[19] It is possible that coefficient estimates on child-specific variables are still biased, if variables are correlated with omitted causes of enrollment age that vary across children *within* households. This concern prompted other authors to identify instrumental variables that varied across children within households (Alderman et al. 2006; Glewwe et al. 2001). I do not pursue a similar strategy in this paper.

[20] Glewwe and Jacoby's (1995) earlier analysis applied ordered probit to a similar dependent variable.

[21] To identify indigenous students, I use language status rather than ethnic self-identification for conceptual and practical reasons. Linguistic development in the non-indigenous language is itself an indicator of readiness for primary schooling conducted in Spanish. Also, such variables are usually more likely to be directly reported for young children in household surveys. See Layton and Patrinos (2006) for estimates of the indigenous population in Latin America. Additional estimates for Bolivia, Chile, and Guatemala are provided in McEwan (2004) and McEwan and Trowbridge (2007).

Additional categories in each country identify students with self-reported dominance in indigenous language, or non-white students in Brazil. Table A1 reports country-specific details on each category.

The Guatemalan and Panamanian surveys include child anthropometrics. From each survey, I extract height-for-age and weight-for-height Z-scores. Each measures the deviation of a child from median in a healthy population of the same gender and biologic age. The former is an indicator of chronic malnutrition, and the latter is an indicator of temporary nutritional deprivation.

The Panamanian and Paraguayan surveys contain an additional indicator of child readiness: whether a child *attended* pre-primary school (most surveys are only suitable for measuring current pre-primary attendance). There are some disadvantages to using this independent variable. Though it presumably captures the degree to which children are adequately prepared for the first-grade, it may also reflect the effects of omitted variables, relating to household or community socioeconomic status.

Household variables include, first, a measure of income per capita. This is obtained by summing monetary labor and non-labor income across household members, and then dividing by the number of household members. Second, I include the years of schooling of each child's mother and father. These variables could reflect parental preferences for schooling, but one suspects that they also capture unmeasured sources of income or wealth. Third, I include the number of children under 21 in the household, to gauge the competing demands for household resources, especially for investments in schooling and health. Fourth, I include three binary indicators of household services: access to a fixed water supply, electricity, and telephone. These variables probably do not have a direct causal interpretation. Rather, they are the best available proxies for family wealth, which theory suggests could explain enrollment delays. They may also proxy local school availability, which is often correlated with community wealth.

To estimate variants of equations (1), I define samples of 6 and 7 year-olds in each country. To estimate equations (2) and (3), I use a larger sample of 6 to 11 year-olds. The sample of 6 and 7 year-olds is no less than 892 (Paraguay), but as high as 3,409 (Argentina) and 14,793 (Brazil). In some samples, the number of observations is smaller for some independent variables, particularly parental schooling, due to non-reported values or non-residence of a parent in the household. To avoid dropping observations in the regression analysis, I code missing values to zero while also including dummy variables that indicate observations with missing observations (e.g. Somers et al 2004). The regression analyses control for all variables, though I do not report coefficients for dummy variables that indicate missing values.

Results

Descriptive Statistics

Table 4 reports, for each country, the weighted mean of independent variables among 6 and 7 year-olds *that attend school*. Alongside each mean is the difference in means between non-attending and attending students (in bold, when the difference is statistically significant at 5%). Dashes indicate that the variable is not available in a particular country's survey.

With some exceptions, non-attending children are more likely to live in rural areas and more likely to belong to an indigenous group or racial minority. In surveys with additional variables, non-attending children have substantially lower height-for-age, though not weight-for-height, and are less likely to have attended pre-primary. A notable exception to these patterns is gender:

except for a large disadvantage for girls in Paraguay, there are few statistically significant differences.

The income gradient is still evident: family income per capita of non-attending students is approximately 28 to 80 percent lower (with the exception of Bolivia). It is matched by lower parental schooling in all countries; larger numbers of children; and diminished access to household services like water, electricity, and telephone. While suggestive, the descriptive data cannot distinguish among potential explanations of delayed enrollment. For that, I turn to multivariate analyses.

Main Regressions

Table 5 reports OLS estimates of equation (1) in each country's sample, in which coefficients can be interpreted as marginal probabilities. In some countries, sample sizes may be insufficient for the precise estimation of coefficients in the presence of collinearity among independent variables. Nonetheless, there are empirical regularities that emerge from the eight regressions.

First, rural residence does not appear to lower attendance probabilities, all else equal, given a consistent pattern of statistically insignificant and/or positive coefficients. The rural disadvantage noted in Table 4 apparently reflects the influence of correlated individual and household variables. This is suggestive that deficiencies in school supply are not driving enrollment delays, since deficiencies in school infrastructure are concentrated in rural areas (McEwan and Trowbridge 2007).

Second, girls are not at a consistent disadvantage and, in Brazil and Nicaragua, appear *more* likely to enter primary school at a younger age. This may reflect parents' perception, in some countries, that boys are relatively less "ready" for school at younger ages, and thus more likely to benefit from delayed enrollment. Consistent with this, McEwan and Shapiro (forthcoming) find that effects of delays on subsequent academic achievement in Chile are roughly one-third larger among boys.

Third, membership in a linguistic or racial minority is consistently associated with lower attendance, even conditional on other measures of socioeconomic status. In Guatemala, Nicaragua, Panama, and Peru, speaking an indigenous language ("Category 2") lowers the probability of attendance between 0.09 and 0.16 (these magnitudes, while large in a practical sense, are statistically insignificant in Nicaragua and Peru). In Brazil, young black children ("Category 2") are 0.07 less likely to attend. It is possible that these effects are capturing the impact of unobserved family income or wealth. It is also possible that they serving as controls for pre-primary school readiness, such as a child's early skills in the non-indigenous language. Most Latin American schools still do not emphasize bilingual instruction, and there are large gaps in test scores between indigenous and nonindigenous children that persist *within* primary schools (McEwan 2004; McEwan and Trowbridge 2007). Thus, enrollment delays may be the best available strategy for indigenous families to bolster children's readiness for school environments that are usually monolingual in Spanish.

Fourth, related indicators of school readiness are associated with early primary attendance. Child height-for-age (but not weight-for-height) is a strong predictor of delays. An additional standard deviation in height-for-age increases attendance probabilities by 0.03 to 0.06. In Panama and Paraguay, pre-primary attendance is correlated with earlier primary attendance. One interpretation is that pre-primary attendance improves student readiness in multiple dimensions (Schady 2006). Another interpretation is that child nutrition and pre-primary attendance are correlated with omitted variables, which I examine more carefully in the next analysis.

Fifth, there is mixed evidence that income and wealth can explain early primary attendance. Household income has no statistically significant links, and the signs of coefficients are not consistent. The number of children under 21 is almost always insignificant, except in Brazil's large sample, though it does have a consistently negative sign. Indicators of household services are strongly correlated with attendance in the three countries with higher enrollment ages: Brazil, Guatemala, and Nicaragua. The results are consistent with binding income or wealth constraints, but they could simply indicate that richer families or communities in these countries are more likely to allow 6 year-olds to violate a minimum age and enroll early.

Additional Regressions from Guatemala and Panama

Figure 3 shows a histogram of self-reported, integer enrollment ages in samples of Guatemalan and Panamanian children, ages 6 to 11. The modal enrollment ages in each sample are 7 and 6, respectively, and the weighted sample means are 7.3 and 6.1. This is roughly consistent with mean estimates from Table 2.[22] Table 6 reports estimates of equations (2) and (3), in order to corroborate and partially extend the previous multivariate analyses. The basic OLS estimates in columns (1) and (4) confirm some previous findings, especially the importance of indigenous status. Height-for-age is an important determinant of enrollment age in Guatemala, while pre-primary attendance is still an important correlate in Panama.

The inclusion of household fixed effects, in columns (2) and (5), absorbs observed and unobserved variables that are constant across families, such as income or access to local school facilities. In column (2), the height-for-age coefficient is almost unchanged, bolstering its causal

[22] The estimates are not strictly comparable. First, the estimates in Table 2 are weighted means of category midpoints rather than integers. Second, the estimates are based on ages calculated on January 1. The self-reported integers are presumably determined in relation to the first day of enrollment or registration, but neither survey form is clear on this matter. Third, the estimates assume right-censoring only for the highest age, whereas the data in Figure 3 reflect right-censoring among younger children.

interpretation. However, two caveats remain. First, a child's earlier enrollment might not be caused by better nutritional status relative to a sibling, but could merely reflect that she is a favored recipient of *any* household resource (note, however, that gender is held constant within households). Second, even if causal, the height-for-age effect could mask distinct channels of influence. The preferred interpretation of empirical researchers has been that nutritional deficiencies, and associated stunting, inhibit readiness for learning. It is also possible that families interpret "readiness" more broadly. For example, primary school attendance often involves long walks through rural areas, and smaller children may be less safe on such journeys.

The Panamanian results are less stable upon the inclusion of household fixed effects. This probably reflects the small amount of within family variation in enrollment age and pre-primary attendance in this country's sample. Finally, the results are not notably sensitive to the estimation method. Columns (3) and (6) report interval regressions that account for a dependent variable that is interval-coded and right-censored for some observations. If anything, the coefficient estimates become slightly stronger.

Discussion

The empirical evidence from Latin America suggests that children's nutrition, linguistic and racial background, and, to a lesser extent, family income and wealth bear the most consistent links to enrollment age. The varied causes imply distinct policy responses. This section reviews evidence on four types of interventions, including (1) early childhood interventions in nutrition, daycare, or preschool; (2) cash transfers to poor families, usually conditional on participation in nutrition, health, and education programs; (3) targeted interventions in primary school quality; and (4) primary school construction.

Early Childhood Interventions

Schady (2006) reviews empirical evidence from Mexico, Brazil, and Ecuador that describes deficits in early childhood outcomes. He concludes deficits appear early, are more intense among children from relatively poorer families, and that this socioeconomic disadvantage becomes more intense among older children (up until the age of kindergarten or primary school enrollment). If enrollment delays partly result from low readiness for primary schooling—especially among poor children—then a compelling policy alternative is to invest in the nutrition, health, and education of very young children.

In Latin America and the Caribbean, the highest-quality evidence on the impact of early childhood interventions is from a Jamaican randomized experiment (Schady 2006). A sample of stunted children, ages 9 to 24 months, was randomly assigned to four groups: food supplementation, home visits by social workers, a combination of the two, and a control group. After two years, all three interventions had positive effects on measures of child development, especially in the combined intervention (Grantham-McGregor et al. 1991). In later childhood, sustained effects were only observed for home visits and the combined treatment, but not food supplementation by itself (Grantham-McGregor et al. 1997; Walker et al. 2000).

Additional non-experimental research on early childhood development has been carried out in Bolivia, Colombia, and Argentina. Behrman et al. (2004) evaluate a Bolivian daycare program that provides child care, nutrition, and education services to families to children between 6 and 72 months of age. Using propensity score matching to form a comparison group, they find effects on motor skills, psychosocial skills, and language acquisition that are concentrated among relatively older children. In a Colombian evaluation, a daycare program with nutritional supplementation produced early gains in child height as well as later school enrollment (Attanasio and Vera-Hernández 2004). Finally, Berlinski et al. (2006) find that cohorts of Argentine children exposed to a large-scale preschool construction project eventually obtain higher test scores in primary schools.

Conditional Cash Transfers (CCTs)

It is possible that enrollment delays are the exclusive result of poverty and related constraints. In such cases, early childhood investments will not necessarily reduce delays (despite other beneficial effects). Instead, cash transfers could alleviate constraints by compensating families for the direct and indirect costs of primary schooling. The accumulated evidence from Latin America, much of it from randomized experiments, suggests that cash transfers, usually conditional on participating in education or health programs, improve school enrollments among primary-aged children (Rawlings and Rubio 2005), although there is less consistent evidence of impacts on delays (Behrman et al. 2005).

Most CCTs require families with very young children (up to the age of 2 or 3) to participate in early childhood programs, including nutrition education for mothers, health center visits, and nutritional supplementation (Rawlings and Rubio 2005; Schady 2006). In this regard, the typical CCT could also promote school readiness in the manner of the center-based programs described above. An evaluation of Mexico's Progresa program found that young participants have better motor skills and fewer socioemotional problems (Gertler and Fernald 2004; Schady 2006). Another Progresa evaluation finds lower probabilities of growth stunting among participating children (aged 12 to 36 months) (Behrman and Hoddinott 2000; Skoufias 2005).

Targeted Interventions in Primary School Quality

In Latin America, compensatory primary school interventions are increasingly targeted at poor or low-achieving schools and students. The interventions include, among others, textbooks and instructional materials, teacher in-service training, remedial tutoring, bilingual instruction, school feeding programs, and specialized multigrade instruction in rural schools.[23] As one much-imitated example, Chile's P-900 program targeted roughly 10 percent of the lowest-achieving primary schools with a package of remedial tutoring and teacher training, leading to substantial increases in early primary test scores (Chay et al. 2005). An increasing number of schools in Bolivia, Guatemala, and other countries with large indigenous populations offer targeted bilingual instruction that is both intended to preserve native languages and speed acquisition of Spanish, though credible evaluation findings are not available (McEwan 2004; McEwan and Trowbridge 2007).

Such interventions are traditionally understood as a means of increasing short-run student achievement, lowering grade repetition, and improving long-run outcomes. Yet, such programs could also lessen the educational or personal costs confronted by children who enroll in primary schools when they are less "ready" for formal schooling. As such, they could improve incentives to enroll on time, although there is only suggestive evidence on this point.[24]

Primary School Construction

[23] For overviews of compensatory education reforms in Latin America, see Winkler (2000), Anderson (2005), and McEwan (forthcoming).

[24] School meals are commonly provided to disadvantaged schools, or students within schools. Even if school meal programs do not directly impact student achievement or nutritional outcomes—and the limited experimental evidence is mixed on this point—they apparently induce families to send children to school regularly to obtain the benefit (Jacoby et al. 1996; Powell et al. 1998; Vermeersch and Kremer 2005). They could improve incentives to enroll on time.

A final policy aimed at reducing delays is the supply of additional primary school infrastructure, presuming that delays are mainly the result of supply constraints. In Latin America, most children have access to a school with at least the early primary grades. Indeed, a common practice in isolated rural areas is to provide a small, single-teacher school that enrolls students of mixed ages and grade-levels (McEwan forthcoming). The school may be of low quality, or offer a limited number of upper grades, but initial *access* is probably not a binding constraint on the initial enrollment decision. The best empirical evidence on this point is that delays persist in urban areas of Latin America with sufficient school infrastructure. For example, Guatemalan regressions from Table 3 in the urban subsamples are similar results to full-sample results, including the importance of indigenous status and child height-for-age in explaining delays.

Conclusions

The phenomenon of delayed primary school enrollment is ubiquitous in Latin America, but has been subjected to little research. This paper described its magnitude, using household surveys from eight counties that facilitate consistent comparisons of enrollment rates among cohorts of young children. It then reviewed theory which suggested that delays may either be a rational response by families to low levels of school readiness, or constrained responses to low incomes, insufficient school supply, or enrollment rules.

Existing literature and the paper's own empirical analysis are consistent with the view that readiness, measured by increased height-for-age, pre-primary attendance, or linguistic background is associated with reduced enrollment delays. Household income and wealth proxies have mixed associations with enrollment delays. The most promising policy alternatives—and the most clearly evidence-based—are the use of direct investments in early childhood nutrition and education, as well as conditional cash transfers with an emphasis on early childhood interventions. The latter policies simultaneously address concerns related to readiness and poverty constraints.

In the future, more complex treatment conditions and randomized evaluations could ascertain whether particular conditionalities are more cost-effective in reducing delays and improving achievement, attainment, and longer-run outcomes. In a similar vein, compensatory policies in primary school, already a costly component of education policy in many countries, could be better targeted to children at risk of early failure in primary school, and better evaluated for their impacts on a similar range of outcomes.

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Table 1: Household surveys and schooling in eight countries

Country	Survey month(s)	Survey year	Survey	Starting age of primary schooling	Starting/ending months of school year
Argentina	5	2000	Encuesta Permanente de Hogares	6	2/12
Bolivia	11-12	2000	Encuesta Continua de Hogares	6	2/11
Brazil	9	2001	Pesquisa Nacional por Amostra de Domicílios (PNAD)	7	3/12
Guatemala	7-12	2000	Encuesta Nacional sobre Condiciones de Vida	7	3/10
Nicaragua	4-7	2001	Encuesta Nacional de Hogares sobre Medición de Nivel de Vida	7	2/12
Panama	8-11	2003	Encuesta de Niveles de Vida	6	3/12
Paraguay	11-12	2002	Encuesta Permanente de Hogares	6	2/11
Peru	5-6	2000	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida	6	4/12

Source: Data in the final two columns are from UNESCO (2006).

Table 2: Net primary enrollment rates, by age

Integer age on January 1	Argentina (urban)	Bolivia	Brazil	Guatemala	Nicaragua	Panama	Paraguay	Peru
3	0	--	0	0	0	0	0	0.001
	(0)	--	(0)	(0)	(0)	(0)	(0)	(0.001)
4	0.009	0.084	0.016	0.001	0.013	0.004	0.183	0.02
	(0.009)	(0.084)	(0.016)	(0.001)	(0.013)	(0.004)	(0.183)	(0.019)
5	0.488	0.58	0.118	0.047	0.342	0.35	0.566	0.162
	(0.479)	(0.496)	(0.102)	(0.046)	(0.329)	(0.346)	(0.383)	(0.142)
6	0.980	0.899	0.702	0.495	0.736	0.923	0.903	0.633
	(0.492)	(0.319)	(0.584)	(0.448)	(0.394)	(0.573)	(0.337)	(0.471)
7	0.993	0.975	0.936	0.772	0.814	0.949	0.939	0.923
	(0.013)	(0.076)	(0.234)	(0.277)	(0.078)	(0.026)	(0.036)	(0.290)
8	0.991	0.979	0.961	0.845	0.922	0.98	0.97	0.943
	(0.007)	(0.004)	(0.025)	(0.073)	(0.108)	(0.031)	(0.031)	(0.020)
9	0.994	0.973	0.970	0.87	0.893	0.974	0.976	0.971
		(0.021)	(0.009)	(0.025)	(0.078)	(0.020)	(0.006)	(0.028)
10	0.996	0.960	0.975	0.846	0.888	0.960	0.979	0.911
			(0.005)	(0.130)			(0.003)	(0.029)
11	0.970	0.912	0.975	0.818	0.764	0.732	0.923	0.566
			(0.025)				(0.021)	
Ages 6-11	0.988	0.950	0.919	0.769	0.837	0.924	0.949	0.822
Mean age of enrollment	6.03	5.98	6.82	7.47	6.67	6.29	5.98	6.85

Notes: Cells report the weighted mean of a dummy variable, equal to 1 if a student attends primary school. Integer ages are calculated on January 1 of each country's survey year. Figures in parentheses are the increase between integer age categories in the net enrollment rate (see text for details).

Table 3: Net primary enrollment rates, among children ages 6 to 7, by quintile of family income per capita

Country	Quintile of family income per capita 1	2	3	4	5	Ratio (5 to 1)	Ratio (4 to 2)
Argentina	0.98	0.99	0.99	1.00	0.99	1.02	1.01
Bolivia	0.96	0.94	0.94	0.98	0.95	1.00	1.03
Brazil	0.81	0.84	0.87	0.89	0.92	1.13	1.05
Guatemala	0.62	0.60	0.71	0.73	0.82	1.33	1.21
Nicaragua	0.71	0.74	0.84	0.89	0.94	1.33	1.20
Panama	0.92	0.93	0.98	0.97	0.99	1.08	1.04
Paraguay	0.91	0.93	0.92	0.96	0.97	1.06	1.02
Peru	0.76	0.81	0.85	0.88	0.85	1.12	1.08

Notes: Cells report the weighted mean of a dummy variable, equal to 1 if a student attends primary school, within samples of 6 to 7 year-olds defined by country and quintiles of family income per capita.

Table 4: Variable means among children ages 6 and 7, by primary school attendance

	Argentina		Bolivia		Brazil		Guatemala		Nicaragua		Panama		Paraguay		Peru	
	Mean	Diff.	Mean	Diff.	Mean	Diff.	Mean	Diff.	Mean	Diff.	Mean	Diff.	Mean	Diff.	Mean	Diff.
Rural	--	--	0.392	0.071	0.175	**0.037**	0.616	**0.109**	0.424	**0.174**	0.469	0.145	0.506	0.097	0.431	0.070
Female	0.487	0.016	0.501	0.029	0.500	**-0.035**	0.473	0.057	0.495	-0.055	0.512	-0.035	0.498	**-0.150**	0.491	-0.028
Indigenous/race																
Category 1	--	--	0.749	-0.013	0.510	**-0.103**	`0.754	**-0.335**	0.960	**-0.062**	0.874	**-0.376**	0.326	-0.094	0.882	**-0.074**
Category 2	--	--	0.248	0.016	0.044	**0.022**	0.216	**0.161**	0.031	**0.070**	0.119	**0.383**	0.124	-0.051	0.118	**0.074**
Category 3	--	--	0.003	**-0.003**	0.442	**0.080**	0.001	-0.000	0.009	**-0.008**	0.007	**-0.007**	0.515	**0.144**	--	--
Category 4	--	--	--	--	0.003	0.001	--	--	--	--	--	--	0.035	0.001	--	--
Height-for-age	--	--	--	--	--	--	-1.436	**-0.654**	--	--	-0.682	**-1.423**	--	--	--	--
Weight-for-height	--	--	--	--	--	--	0.373	-0.038	--	--	0.347	0.321	--	--	--	--
Attended pre-prim.	--	--	--	--	--	--	--	--	--	--	0.795	**-0.479**	0.777	**-0.362**	--	--
Log income p/c	4.769	**-0.617**	4.727	0.036	4.688	**-0.304**	5.372	**-0.520**	5.827	**-0.677**	4.364	**-0.795**	11.740	-0.275	4.715	**-0.307**
Father's schooling	9.640	**-3.568**	7.647	**-2.372**	6.635	**-0.808**	4.609	**-2.183**	5.130	**-2.820**	8.051	**-1.811**	7.099	**-2.041**	8.993	**-1.037**
Mother's schooling	9.815	**-2.628**	5.758	**-1.934**	6.871	**-0.875**	3.315	**-1.580**	4.675	**-2.437**	8.376	**-3.710**	6.528	**-2.016**	7.229	**-0.993**
Children under 21	3.341	**1.257**	3.912	0.410	2.908	**0.437**	4.517	**0.557**	4.435	**0.718**	3.797	**1.653**	4.078	**1.516**	3.361	**0.331**
Water	0.975	-0.004	0.674	-0.073	0.779	**-0.083**	0.711	**-0.147**	0.587	**-0.295**	0.866	**-0.182**	0.468	-0.066	0.687	**-0.097**
Light	0.993	-0.010	0.660	-0.097	0.953	**-0.061**	0.762	**-0.238**	0.691	**-0.337**	0.736	**-0.327**	0.898	**-0.178**	0.717	**-0.089**
Telephone	--	--	0.177	**-0.097**	0.442	**-0.126**	0.114	**-0.068**	0.083	**-0.076**	0.207	**-0.169**	0.095	**-0.054**	0.157	-0.006

Notes: Cells in the "Mean" columns report weighted means for each background variable, in country-specific samples of students ages 6 and 7 that attend primary school. Cells in the "Difference" columns report the difference between the mean of non-attending and attending students. Means for each variable mean exclude students with missing cases; sample sizes for each variable are reported in Table A1. Bold values indicate that the difference is statistically significant at 5% (standard errors in hypothesis tests are adjusted for clustering within families). Integer ages are calculated on January 1 of survey years.

Table 5: Determinants of primary school attendance among children ages 6 and 7

	Argentina	Bolivia	Brazil	Guatemal.	Nicaragua	Panama	Paraguay	Peru
Rural	--	0.014	0.065**	0.054	0.077*	0.035	0.025	0.039
		(0.026)	(0.013)	(0.034)	(0.037)	(0.019)	(0.026)	(0.042)
Female	-0.001	-0.006	0.020**	-0.045	0.052*	0.004	0.031	0.018
	(0.006)	(0.018)	(0.007)	(0.024)	(0.025)	(0.015)	(0.019)	(0.030)
Indigenous/race								
Category 2	--	0.020	-0.067**	-0.144**	-0.155	-0.098**	0.022	-0.087
		(0.023)	(0.018)	(0.031)	(0.086)	(0.034)	(0.029)	(0.054)
Category 3	--	0.028	-0.020**	-0.200	0.137**	0.037	0.006	--
		(0.033)	(0.007)	(0.183)	(0.044)	(0.051)	(0.031)	
Category 4	--	--	-0.052	--	--	--	0.043	--
			(0.055)				(0.065)	
Height-for-age	--	--	--	0.064**	--	0.025**	--	--
				(0.010)		(0.008)		
Weight-for-height	--	--	--	-0.004	--	-0.006	--	--
				(0.012)		(0.007)		
Attended pre-prim.	--	--	--		--	0.113**	0.114**	--
						(0.026)	(0.033)	
Log income p/c	-0.000	-0.000	0.002	-0.001	0.013	0.002	-0.001	0.013
	(0.002)	(0.009)	(0.003)	(0.008)	(0.009)	(0.004)	(0.003)	(0.014)
Father's schooling	0.002*	0.007*	0.001	0.011*	0.013**	-0.003	0.001	0.003
	(0.001)	(0.003)	(0.001)	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)
Mother's schooling	0.000	0.002	0.003*	0.001	0.010*	0.004	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.005)	(0.004)	(0.003)	(0.004)	(0.005)
Children under 21	-0.004	-0.003	-0.012**	-0.007	-0.005	-0.001	-0.014	-0.014
	(0.003)	(0.006)	(0.003)	(0.007)	(0.007)	(0.006)	(0.007)	(0.010)
Water	-0.009	0.005	0.041**	0.068*	0.084*	-0.012	-0.020	0.047
	(0.013)	(0.022)	(0.011)	(0.033)	(0.034)	(0.030)	(0.028)	(0.041)
Light	0.014	0.002	0.126**	0.144**	0.145**	-0.032	0.097*	0.050
	(0.026)	(0.027)	(0.021)	(0.032)	(0.037)	(0.026)	(0.045)	(0.045)
Telephone	--	0.011	0.034**	-0.020	0.016	0.002	0.024	-0.067
		(0.021)	(0.008)	(0.045)	(0.035)	(0.014)	(0.031)	(0.044)
Number of obs.	3,409	1,056	14,793	2,289	1,354	1,221	892	1,021
Adj. R^2	0.009	0.009	0.028	0.210	0.132	0.171	0.095	0.012
Weighted mean of dependent variable	0.987	0.937	0.820	0.634	0.778	0.937	0.921	0.775

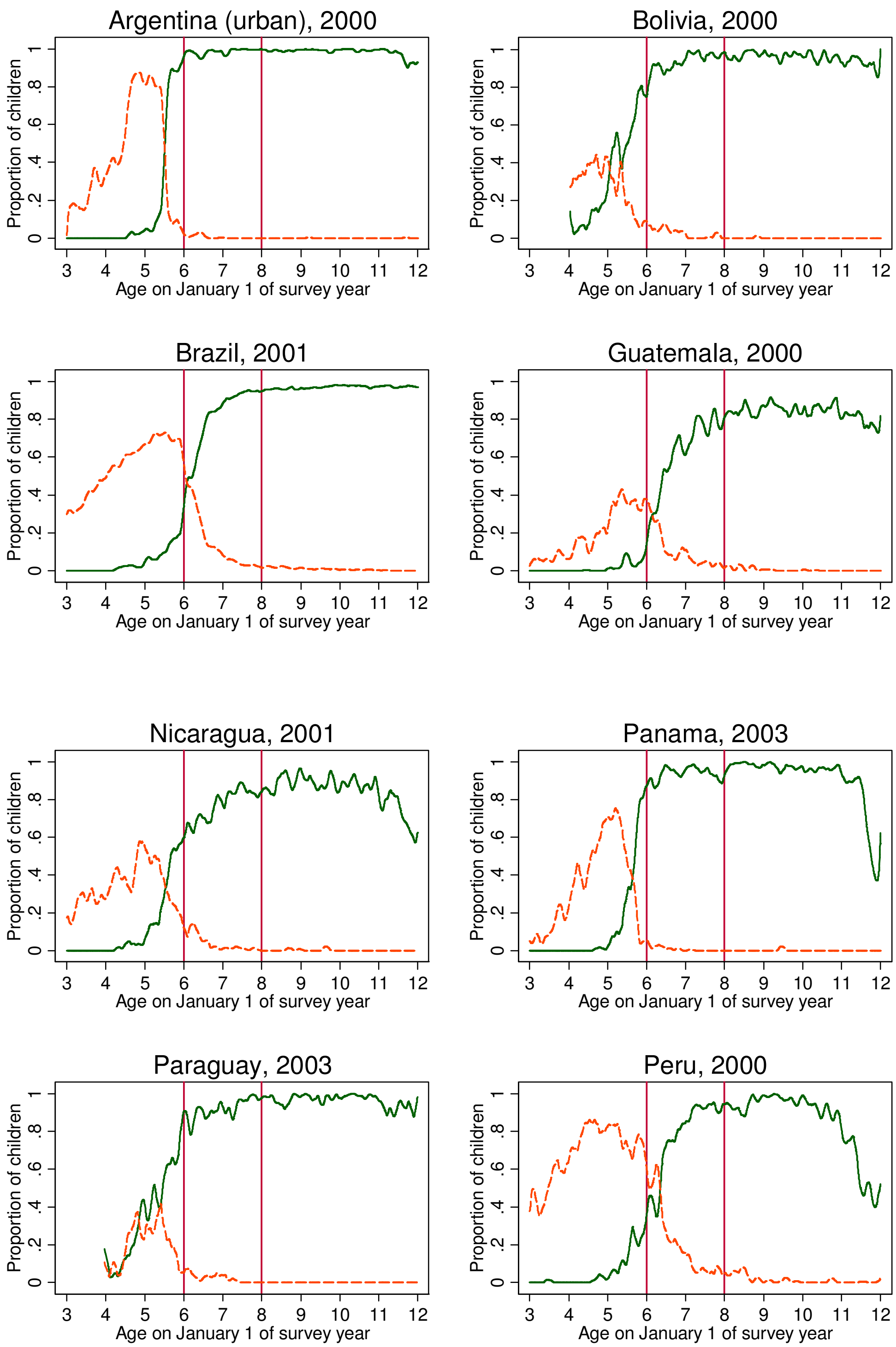
Notes: ** (*) indicates statistical significance at 1% (5%). Standard errors, adjusted for clustering within families, are in parentheses. In addition to the reported control variables, each regression includes a constant and dummy variables that indicate missing values of reported independent variables. Regressions are weighted. The sample includes students aged 6 and 7 (integer ages are calculated on January 1 of survey years).

Table 6: Determinants of first-grade age of enrollment, Guatemala and Panama

	Guatemala			Panama		
	(1)	(2)	(3)	(4)	(5)	(6)
Rural	-0.014	--	-0.039	0.035	--	0.038
	(0.051)		(0.055)	(0.026)		(0.028)
Female	-0.044	-0.067	-0.010	-0.029	0.005	-0.031
	(0.038)	(0.051)	(0.041)	(0.019)	(0.025)	(0.021)
Indigenous						
Category 2	0.308**	--	0.366**	0.164**	--	0.186**
	(0.058)		(0.065)	(0.048)		(0.050)
Category 3	1.277**	--	1.795**	0.092	--	0.096
	(0.306)		(0.497)	(0.151)		(0.153)
Height-for-age	-0.108**	-0.102**	-0.125**	-0.017	-0.004	-0.018
	(0.018)	(0.029)	(0.020)	(0.011)	(0.016)	(0.012)
Weight-for-height	0.029	-0.026	0.027	-0.004	-0.033	-0.004
	(0.017)	(0.029)	(0.020)	(0.010)	(0.018)	(0.010)
Attended pre-primary	--	--	--	-0.116**	0.006	-0.126**
				(0.027)	(0.049)	(0.028)
Log income p/c	-0.019	--	-0.019	0.004	--	0.003
	(0.014)		(0.015)	(0.006)		(0.006)
Father’s schooling	-0.026**	--	-0.030**	-0.002	--	-0.003
	(0.007)		(0.007)	(0.004)		(0.005)
Mother’s schooling	-0.030**	--	-0.031**	-0.011**	--	-0.011**
	(0.007)		(0.007)	(0.004)		(0.004)
Children under 21	0.019	--	0.020	-0.001	--	-0.001
	(0.012)		(0.013)	(0.006)		(0.007)
Water	-0.200**	--	-0.237**	-0.021	--	-0.016
	(0.052)		(0.058)	(0.045)		(0.048)
Light	-0.448**	--	-0.530**	-0.012	--	-0.012
	(0.059)		(0.066)	(0.035)		(0.037)
Telephone	0.088	--	0.110	0.023	--	0.026
	(0.063)		(0.066)	(0.027)		(0.028)
Estimation method	OLS	OLS	Interval	OLS	OLS	Interval
Household fixed effects	No	Yes	No	No	Yes	No
Observations	6,183	6,183	6,183	3,464	3,464	3,464
Adjusted R-squared	0.197	0.797	--	0.063	0.785	--
Weighted mean of dependent variable		7.32			6.10	

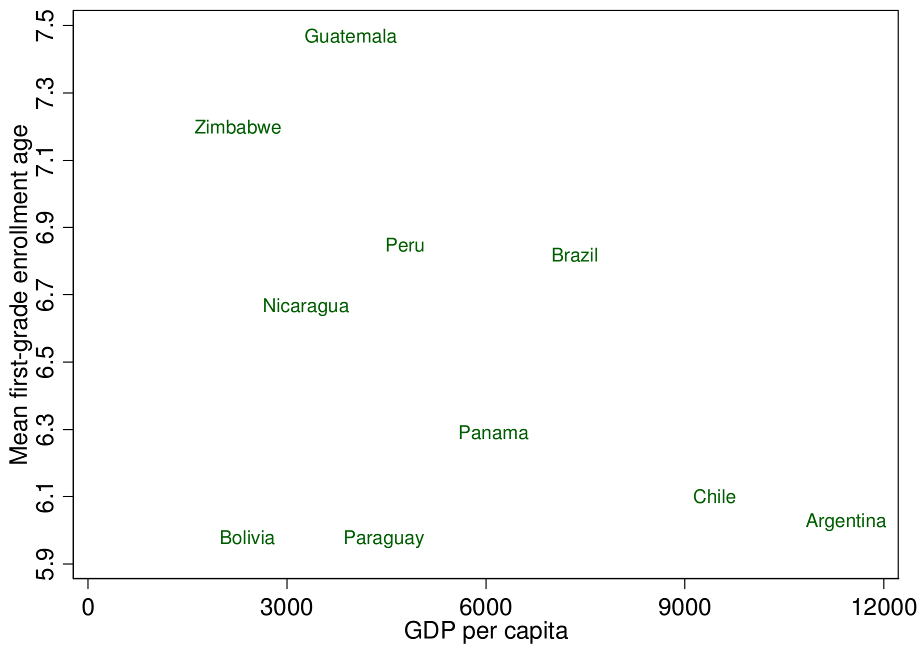
Notes: ** (*) indicates statistical significance at 1% (5%). Standard errors, adjusted for clustering within families, are in parentheses. In addition to the reported control variables, each regression includes a constant and dummy variables that indicate missing values of reported independent variables. Regressions are weighted. The sample includes students aged 6 to 11 (integer ages are calculated on January 1 of the survey year). See text for details on estimation method.

Figure 1: Proportion of students enrolled in primary and pre-primary, by decimal age



Notes: Solid lines indicate primary attendance, and dotted lines indicate pre-primary attendance. Lines indicate fitted values from local linear regressions (bandwidth=0.03); see text for details.

Figure 2: Mean first-grade enrollment age and GDP per capita



Source: Enrollment ages are from Table 2, McEwan and Shapiro (2006), and Alderman et al. (2006). GDP per capita is the four-year mean (2000 to 2003) of GDP per capita, purchasing power parity adjusted, constant 2000 prices, from the World Development Indicators.

Figure 3: Self-reported age of first-grade enrollment among students aged 6 to 11

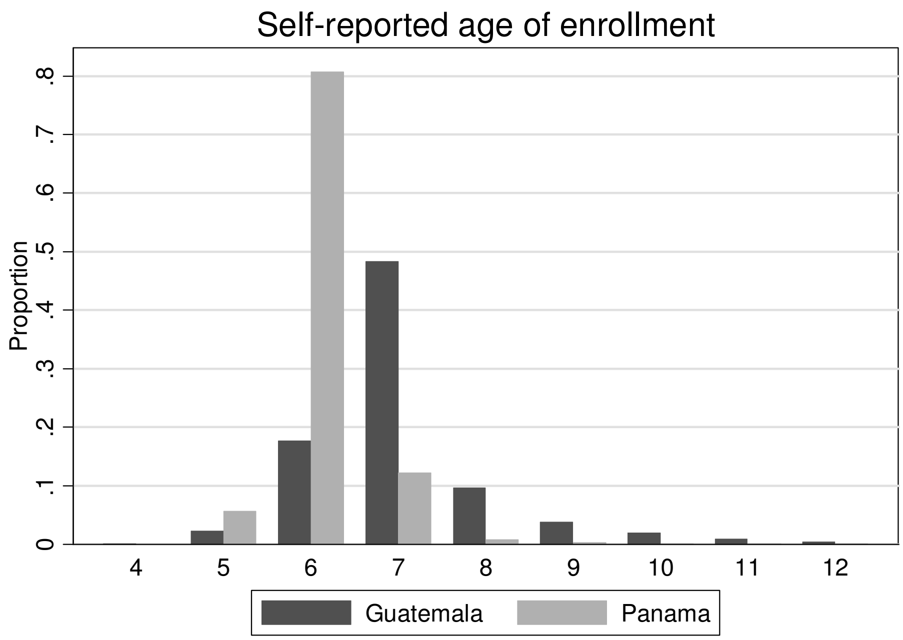


Table A1: Selected variable definitions

	Attends pre-primary	Attends primary	Indigenous/race	Other variable(s)
Argentina	Attends school (P55) that is *pre-escolar* (P56)	Attends school (P55) that is *primario* (P56)	n/a	n/a
Bolivia	Attends school (section 4, question 4) that is *educación pre-escolar (pre-kinder/kinder)* (section 4, question 5)	Attends school (section 4, question 4) that is *primaria* (section 4, question 5)	"What is the language in which you learned to speak in your youth?" Cat.1: Spanish Cat. 2: Indigenous language, including Quechua, Aymara, Guaraní. Cat. 3: Other	n/a
Brazil	Attends *escola* or *creche* (section 6, question 2) that is *creche* or *pré-escolar* (section 6, question 3).	Attends *escola* or *creche* (section 6, question 2) that is *regular/supletivo de ensino fundamental o de 1 grau* (section 6, question 3).	"The color or race of … is" (section 4, question 4). Cat. 1: White Cat. 2: Black Cat. 3: Brown Cat. 4: Other	n/a
Guatemala	If 0-6 years-old, attends *guardería, prekinder, kinder, párvulos,* or *preparatoria* (ch. 7, part A, question 1). If >6 years-old, attends school (ch. 7, part B, question 5) that is *preparatoria* (ch. 7, part B, question 6).	If 0-6 years-old, attends *escuela primaria* (ch. 7, part A, question 1). If >6 years-old, attends school (ch. 7, part B, question 5) that is *primaria* (ch. 7, part B, question 6).	"What is the maternal language of … ?" (ch. 5, part B, question 1) Cat. 1: Spanish Cat. 2: Indigenous languages, mostly Mayan Cat. 3: Other	Height-for-age and weight-for-height Z-scores are calculated with EpiInfo and CDC/WHO 1978 reference population.
Nicaragua	If 0-6 years-old, attends *comedor infantil/CICO*, *CDI/guardería*, or *preescolar* (section 4, part A, question 2). If >6 years-old, attends school (section 4, part B, question 19) that is preescolar (section 4, part B, question 21)	If 0-6 years-old, attends *escuela* (section 4, part A, question 2). If >6 years-old, attends school (section 4, part B, question 19) that is *primaria* (section 4, part B, question 21)	"What is the language that … speaks since their youth?" (section 2, question 7) Cat. 1: Spanish Cat. 2: Miskito Cat. 3: Other	n/a
Panama	If 0-5 years-old, attends *lactante, maternal, parvulario, prekinder, prejardin, kinder, jardin,* or other preprimary (section 5, part A, question 1). If >5 years-old, attends *pre-escolar* (section 5, part C, question 22)	If 0-5 years-old, attends *escuela primaria* (section 5, part A, question 1). If >5 years-old, attends *primaria* (section 5, part C, question 22)	"What is the maternal language of … ?" (section 5, question 13) Cat. 1: Spanish Cat. 2: Indigenous language Cat. 3: Other	Height-for-age and weight-for-height Z-scores are reported in public-use data files Attended *preescolar*, including *maternal, parvulario, prekinder, kinder* (section 5, part C, question 18).
Paraguay	Attends *pre-escolar*	Attends *primario*	"What language does …	Attended *preescolar* or

	(section 3; question 7)	*(educación escolar básica)* (section 3; question 7)	speak in the house the majority of the time?" (section 3, question 1): Cat. 1: Spanish; Cat. 2: Guaraní and Spanish; Cat. 3: Guaraní Cat. 4: Other	another kind of pre-primary (section 3, question 6)
Peru	Attends a school that is *inicial* or *transicíon* (S3P6a)	Attends a school that is *primaria* (S3P6a)	"What is the mother tongue that … speaks?" (S1P7) Cat. 1: Spanish Cat. 2: Indigenous language, including Quechua, Aymara, others Cat. 3: Other	n/a

Table A2: Unweighted descriptive statistics for children ages 6 to 11

	Argentina		Bolivia		Brazil		Guatemala		Nicaragua		Panama		Paraguay		Peru	
	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.	Mean (s.d.)	Obs.
Attends primary	0.986	10,183	0.950	3,201	0.916	43,364	0.766	6,366	0.817	3,919	0.922	3,468	0.950	2,738	0.828	2,947
Age of enrollment	--		--		--		7.295	6,183	--		6.146	3,464	--		--	
							(1.301)				(0.568)					
Rural	--		0.465	3,201	0.166	43,364	0.624	6,366	0.509	3,919	0.572	3,468	0.523	2,738	0.420	2,947
Female	0.490	10,183	0.494	3,201	0.498	43,364	0.485	6,366	0.504	3,919	0.484	3,468	0.485	2,738	0.491	2,947
Indigenous/race																
Category 1	--		0.743	3,201	0.452	43,358	0.685	6,126	0.944	3,919	0.801	3,468	0.268	2,738	0.902	2,947
Category 2	--		0.252	3,201	0.048	43,358	0.313	6,126	0.043	3,919	0.189	3,468	0.121	2,738	0.097	2,947
Category 3	--		0.005	3,201	0.496	43,358	0.002	6,126	0.014	3,919	0.010	3,468	0.570	2,738	0.001	2,947
Category 4	--		--		0.004	43,358	--		--		--		0.041	2,738	--	
Height-for-age	--		--		--		-1.733	5,945	--		-0.882	3,350	--		--	
							(1.196)				(1.259)					
Weight-for-height	--		--		--		0.387	3,933	--		0.458	2,200	--		--	
							(1.050)				(1.178)					
Attended pre-prim.	--		--		--		--		--		0.658	3,467	0.662	2,738	--	
Log income p/c	4.637	9,739	4.648	3,052	4.635	42,317	5.178	6,187	5.641	3,816	4.203	2,642	11.634	2,499	4.678	2,923
	(0.958)		(1.494)		(1.072)		(1.524)		(1.222)		(1.269)		(1.251)		(1.193)	
Father's schooling	9.295	7,722	7.011	2,570	6.563	27,686	3.712	4,831	4.085	2,506	7.158	2,287	6.610	1,889	8.326	2,184
	(3.769)		(4.530)		(3.851)		(4.324)		(4.188)		(4.314)		(3.690)		(4.634)	
Mother's schooling	9.434	8,700	5.237	2,836	6.734	33,825	2.584	5,438	3.873	2,902	7.256	2,625	6.283	2,143	6.632	2,328
	(3.881)		(4.563)		(3.757)		(3.852)		(4.045)		(4.545)		(3.656)		(4.734)	
Children under 21	3.569	10,183	4.054	3,201	3.057	43,364	4.781	6,366	4.743	3,919	4.142	3,468	4.337	2,738	3.675	2,947
	(1.877)		(1.825)		(1.682)		(2.073)		(2.338)		(2.552)		(2.264)		(1.685)	
Water	0.967	10,183	0.626	3,201	0.754	43,364	0.688	6,366	0.477	3,919	0.837	3,468	0.448	2,738	0.656	2,947
Light	0.988	10,183	0.622	3,201	0.946	43,364	0.672	6,366	0.580	3,919	0.673	3,468	0.892	2,738	0.671	2,947
Telephone	--		0.152	3,200	0.436	43,351	0.097	6,366	0.052	3,919	0.174	3,468	0.104	2,738	0.159	2,947

Note: Standard deviations, in parentheses, are not reported for dummy variables. The sample includes children with integer ages of 6 to 11 (estimated on January 1 of the survey year). Sample sizes are smaller for some variables because of missing values.